Using remote sensing to understand the interdependency between water supply and actual evapotranspiration

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0330: Using remote sensing to understand the interdependency between water supply and actual evapotranspiration

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This proposal is incomplete. The PIs are not versed in SEBAL, the primary technique for determining ET (satellite). Only a private company is. The project proposal writers are not aware of LSAT 7 problems which makes one question their understanding of satellite technology, consequently, there will be no guarantees that images will be available for the project. The mapping and understanding of grower responses will be difficult and the PIs don’t seem to understand this.

Additional Comments:

This proposal is incomplete. The PIs are not versed in SEBAL, the primary technique for determining ET (satellite). Only a private company is. The project proposal writers are not aware of LSAT 7 problems which makes one question their understanding of satellite technology, consequently, there will be no guarantees that images will be available for the project. The mapping and understanding of grower responses will be difficult and the PIs don’t seem to understand this.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The external technical reviewers made a number of substantive comments regarding this proposal, including comments regarding the availability of the necessary satellite data and the mapping and understanding of grower responses. These were considered serious concerns by the panel. In addition, the panel had serious concerns regarding the extent to which natural vegetation would be analyzed by this project as proposed, and regarding the principal investigators' capability to perform the proposed work as the principal investigators are not versed in SEBAL (just the consulting firm). This project is just an algorithm and has little potential for theory developemnt and scientific advancement.

Technical Review #1

proposal title: Using remote sensing to understand the interdependency between water supply and actual evapotranspiration

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objectives of this project are to measure: 1. the effect of different hydrologic years on actual water consumption by crops and other vegetation, and 2. grower response to water availability through the use of two metrics (Kc crop and Ks crop stress co−efficients). The underlying hypothesis is that Ks crop stress will vary with wet and dry years. The objectives are stated clearly. The idea is very important and would be a great service to understanding how much water is used in certain cropping systems in Fresno and Kern Counties.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**It is unclear how much of this research has been done before, where, when, and who was involved− or if any other proposals are out there by the authors to do similiar projects− it seems as if there are, but it would be nice to document some history in this field.** **The conceptual model is clear, with two basic steps,**

	but there is a logical inconsistency. The first step is clear and consistent: estimate evapotranspiration (ET) by crops over wet and dry years. The second step is clear as well: measure grower response through the use of two metrics, Kc crop and Ks crop stress, by using the ET estimates from step 1. However, just because you now have a measure of Ks crop stress does not equate to understanding grower response. There is no mention as to how grower response will be measured (temporal changes in Ks? what type of metric? specifically? this "grower response" idea is vague, what are the variables involved? can you really measure this?). **It seems that the entire connection to CALFED program's goals, as stated in this RFP, is that once the grower response is known/understood, then better water management decisions can be made. This depends on the actual measure of grower response and this methodology is not clearly articulated. Also, the impact of this project on the other program goals, i.e. key species, the Bay, etc. is really not mentioned and is only by side-effect if it had been mentioned.**
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed except for the linkage between understanding Kc crop stress and understanding grower response. There is really no approach specifically documented as to how knowing the Kc values across the time sequences will really help understand grower response in terms of water use. What is the**

	relationship? Is it directly linear, with no complicating variables? Is there a time lag, what are the growers' perceptions? How do previously stressed crops alter potential in the future– there's some dependency right? How do the large number of possible steps between Ks and grower response affect what is actually witnessed via remote sensing? This needs to be ironed out, or at least addressed. **The project seems to replicate a proven method (SEBAL algorithm) within the context of a new example. There are probably other examples of this approach, although the history and previous uses were under–discussed by the authors, and it won't be anything all that new.** **However, the generated information should prove extremely useful to decision makers.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The proposal could be better documented with regards to previous uses of the SEBAL algorithm, but it is convicing that the proposal is technically feasible. The likelihood of success is very good, except for getting a really good fix on the realities of grower response. The scale of the project is quite appropriate and should fit well with the authors in both location and their abilities.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**There is no mention of monitoring in terms of pre−post comparisions or quality control, except for a brief statement about how the SEBAL algorithm makes atmospheric correction unnecessary.** **This proposal could be made much better by including some mention of quality control in terms of the Landsat data, the procedures that will be used, stakeholder dissemination, etc.. Perhaps, the inclusion of some peer−monitoring, or a consulting round table, or an internal review process, etc. could help make sure the project stays on track. The authors mention a stakeholder workshop to be held at the end of the project, but meetings like this throughout the term of the project would make it look even stronger in terms of reviewing, quality control, etc.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products of the project are likely to be excellent. Very clear, very simple: GIS layers of evapotranspiration, Kc and Ks co−efficients for multiple crop types and spatial areas/fields/cities/water districts/etc.** **These products will be very valuable to water managers.**
Rating	

	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors should be capable to carry out the project with no problems. I am convinced that the work by SEBAL North America should be extremely high-quality. The rest is mostly statistical analysis and GIS manipulation, which is well within the abilities of the authors. The infrastructure and support appears to be there.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is very reasonable. In fact, I would argue that the authors could have asked for more money, in particular for the portions of work by Cassell and Yang.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Overall, the project is very good. It would be extremely useful to water managers in the Fresno area. However, it

	could be made into a stronger idea, albeit at a larger scale of work, if there was some type of component that made the evapotranspiration data layers and the Ks data available to water managers real-time, perhaps via an internet site. This could be eventually a large-scale model for California agricultural water management. I know that this is not the intent of this proposal, but it is something to think about in terms of the larger picture and the future potential of this kind of work. **This general direction of work is very needed, extremely useful in terms of the final products, but the proposal does suffer in a few areas. In particular, it seems difficult to fully deliver the second objective in terms of mapping/understanding grower response- it is a little oversold. Still, even just having the crop stress Ks known (for hydrologically different years) for such a large area would be a very nice advancement for water managers.** **This proposal could also be refined in terms of quality-control and the larger justification/relationship to CALFED's goals in this Request for Proposals program. Simply put, there a few loose ends. Overall, however, it is very good.**
Rating	**very good**

Technical Review #2

proposal title: Using remote sensing to understand the interdependency between water supply and actual evapotranspiration

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Yes, the goals, objectives and hypotheses are clearly stated and internally consistent. Simulation of actual ET (ETact) is an important component for water balance analysis, however an algorithm to accurately simulate is yet to develop. Several algorithms are available and have been used in last decades. The advantages and disadvantages of the proposed application are not outlined, and the improvements are also not reported.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is not being considered a significant advancement relative to the existing knowledge. The proposal clearly outlines the concept and methodology. The proposed work can be tested for a small−scale project.**
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed and appropriate for meeting the objectives, and is feasible. The temporal scale of the results is high which may not suitable in many applications. This is an application of an algorithm, and there are many such algorithms available. The information might be used for decision−making process indirectly.**
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is documented sagaciously and concisely. The approach is technically feasible. The project might be a successful, as the team has strong multidisciplinary research background.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The approach does not outline any pre and post monitoring design, however, this model shall be calibrated with the observed data to verify model performances. It is not distinct if the results from the approach will be compared with those from some other models or observed data to check the accuracy of**

	the approach.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The results from the project may be used in decision-making process indirectly. The project outcome may not likely contribute to the larger data management system.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team has moderate publication record, even though they may capable of performing this project. The institutions may have research infrastructure to accomplish this project.
Rating	good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The proposed budget is adequate and reasonable.
Rating	

	very good

Overall

Provide a brief explanation of your summary rating.

Comments	**The project uses an algorithm to simulate actual ET and its relationship with hydrologic year type. The project is an application of an algorithm, and has little potential for theory development and advancement. From the proposal, it is not distinct how the algorithm will perform for the proposed domain and if the results obtained from the algorithm will be compared with those form any other existing model or observed data.**
Rating	**good**

Technical Review #3

proposal title: Using remote sensing to understand the interdependency between water supply and actual evapotranspiration

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives are clearly stated and internally consistent but there is insufficient development of the implications of inferring seasonal water ET from a very limited range of observations. Furthermore, it is not clear from the hypotheses that the primary staff understands the limitations of the remote sensing technology. However, the fundamental idea--assessing ET remotely-- is timely and important.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**There is insufficient justification in evidence, perhaps because the primary staff are not experts in the SEBAL algorithm or remote retrieval of ET.**
Rating	**fair**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach as outlined in the proposal is fundamentally flawed on several counts. First, the tone of the proposal assumes that 18 Landsat scenes per year for three years are available for use in the project. This is simply not the case unless the SEBAL algorithm can magically remove obscuring clouds to reveal surface conditions. A quick look at glovis.usgs.gov reveals that while WRS−2 42/35 has a remarkable number of low cloud scenes available, especially in 2002, there is nowhere near 54 scenes available from ETM+ prior to the SLC failure in 5/2003. Second, the proposal specifies the need for 60m thermal resolution, which is only available through the Landsat 7 ETM+ sensor. But there is no awareness that the sensor underwent a major instrumental failure that severely affects the spatial integrity of the imagery. While SLC−off scenes are available (cf. glovis), the pervasiveness of the gaps in the imagery will restrict the inferential ability of the project substantially. Yes, Landsat 5 TM is still limping along but its thermal resolution is 120m. Third, no detail is provided on how the selection of hydrologic years will proceed, yet this is something that could have easily been done in advance of the proposal submission. Fourth, under the critical task #3 there are two vague phrases that veil the crux of the project goal: "The Kc and Ks values will be connected over time to provide seasonal Kc and Ks functions. These functions will be compared to functions published by the University of California and others and across hydrologic year types." How will these values be "connected over time" and how will these functions be "compared"? This is where the new important science should happen but there is no indication of methods. Fifth, the issue of maps of land use land cover is not trivial. What is the age and heritage of the DWR LULC map? How will the specific crops be classified? How will the accuracy**

	assessment on the classification be done? What is the interannual variability of crop areas and rotations? None of these important questions are addressed by the proposal.
Rating	**poor**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is not fully documented and therefore technical feasibility cannot be assessed. Given what is said and what is not said, I judge the likelihood of success to be low. Yes, the available scenes can be processed with the SEBAL algorithm but that is only the beginning of the science.**
Rating	**poor**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Implicit in the proposal is the potential for monitoring. However, there are many issues that are left unstated.**
Rating	**poor**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The specific description of the deliverables is thin and vague.**

Rating	**poor**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**This is a difficult question to address because most of the funding and the work goes to a subcontractor and there is no detail about the qualifications of the principal funded staff member at the subcontractor nor its infrastructure.**
Rating	**poor**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is troubling in several respects. First, 70% of the total goes to the subcontractor, 20% of the total goes to the University for indirect costs, and 10% goes to the primary staff at the University. Second, it looks like the indirect cost calculation is incorrect. The CSU-Fresno grant guidelines specify that full indirect is assessed against the first $25,000 of each subcontract, regardless of its duration. That would translate into only $10,000 assessed against the $350,900 that is targeted to the subcontractor. However, the proposal contains indirect costs assessed against each task at the rate of 20% for a total of $70,180. Either the grants office dropped the ball in review this budget or something near criminal is going on. Third, Landsat imagery is available through AmericaView California (at UC Davis)**

	at a discount and the SLC-off imagery, if that is to be used, is much less (~$275/scene). Fourth, there is no explanation of why and what is involved in the processing cost of $4800/scene by the subcontractor.
Rating	**poor**

Overall

Provide a brief explanation of your summary rating.

Comments	**The project idea is interesting but the details are missing, the scope is too large, and the budget is way out of line. If there is interest in the panel to fund something kind of capability, I would suggest a 1 year pilot project building on the 2002 scenes already available and a modest budget of less than $100K. More interesting would be a proposal that would combine the infrequent "pearls" of ETM+ with the monitoring capability of MODIS.**
Rating	**fair**